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ABSTRACT

This study assessed the impact of family childhood income on completed years of schooling using fixed effects techniques to eliminate biases associated with omission of unmeasured family characteristics. It also examined the importance of timing of family income, estimating models that related years of completed schooling to average levels of parental income in early childhood, early middle childhood, preadolescence, and adolescence. Data came from the Panel Study of Income Dynamics (PSID), a longitudinal household survey. This study used sibling data on 1,364 families with children born between 1968-76 and present in the PSID between birth and age at least 20 years. The outcome measure, completed years of schooling, was examined when the child was age 20 years. The income measure was the total pre-tax income of all family members, inflated to 1993 price levels. Results indicated that family income during early childhood positively impacted children's educational attainment. The magnitude of the effect suggested that a 2.7-fold increase in parental income when the child was age 0-4 years led to an increase of about half a year of schooling. Income during adolescence had a positive, but less robust, effect. (Contains 21 references.) (SM)

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Using Sibling Samples to Assess the Effect of Childhood Family Income on Completed Schooling*

JCPR Working Paper 168

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Using Sibling Samples to Assess the Effect of Childhood Family Income on Completed Schooling

Abstract

We assess the impact of stage-specific family childhood income on completed years of schooling using fixed effects techniques to eliminate biases associated with the omission of unmeasured family characteristics. Sibling data from the Panel Study of Income Dynamics (PSID) indicate that family income during early childhood has a positive effect on children's educational attainment. The magnitude of the effect suggests that a 2.7-fold increase in parental income when the child is 0-4 years old leads to an increase of about half a year of schooling. We find that income during adolescence has a positive but less robust effect.

Using Sibling Samples to Assess the Effect of Childhood Family Income on Completed Schooling

I – Introduction

Although many studies have examined the association between family income and child development (Haveman and Wolfe, 1995; Mayer, 1997), at least two issues remain to be settled. First, a persistent concern with much of the existing literature is that the estimated effect of income might be spurious, caused by the mutual association that parental income and children outcomes share with unmeasured "true" causal factors, such as parental ability, diligence, mental health or preferences. Suppose, for example, that the ability of parents is a key ingredient for children's success and that measures of parental ability were not included in the models. Since a high level of ability is likely to make parents more successful in the labor market as well as to have children with high cognitive skills (e.g. through genes or enriched home environments), the absence of adjustments for differences in parental ability may produce a serious overstatement of the role income plays in causing children's success.

Second, little is known about the relevance of the timing of economic conditions during childhood. With one exception (Duncan et al., 1998), studies focusing on adolescent and young adult outcomes such as completed schooling have not been able to study the importance of family income in early childhood. Thus, whether poverty in early childhood has important effects on completed schooling remains an open question.

This paper uses whole-childhood data from the Panel Study of Income Dynamics (PSID) to assess the effect of family income on children's completed schooling. It makes two contributions to the literature linking family income to child development. First, it uses fixed effect techniques to eliminate the biases associated with the omission of unmeasured persistent

family characteristics. Second, it looks at the importance of the *timing* of family income by estimating models that relate years of completed schooling to average levels of parental income in early childhood, early middle childhood, pre-adolescence, and adolescence.

The paper is organized as follows: Section II summarizes the literature on the effects of family childhood income on child development. Section III briefly describes the theoretical framework behind our empirical work. Section IV provides the empirical specification, whereas Section V presents a brief description of the data used in this study. Section VI and VII present our results and extensions, and finally, Section VIII provides a summary and discussion of the results.

II – Background

Whether high rates of child poverty affect child development remains a controversial issue (Haveman and Wolfe, 1995; Mayer, 1997; Brooks-Gunn and Duncan, 1997; Blau, 1999). Haveman and Wolfe's (1995) review of the literature through the early 1990s concludes that while many studies find positive and statistically significant associations between income and completed schooling, the effects sizes tend to be small, with the range of elasticities from .02 to .20 (p. 1856).

A more recent and refined look at links between poverty and development is Duncan and Brooks-Gunn's (1997) coordinated efforts involving 12 groups of researchers working with 10 different developmental data sets, most of which offer longitudinal measurement of parental family income as well as measurements of the achievement, behavior or health of individuals at various points in life. Taken as a whole, the results suggest that family income may have substantial but decidedly selective associations with children's attainments. The selective nature

of effects included: i) family income had much larger associations with measures of children's ability and achievement than with measures of behavior, mental health and physical health; ii) family economic conditions in early childhood appeared to be more important for shaping ability and achievement than did economic conditions during adolescence; and iii) the association between income and achievement was non-linear, with the biggest impacts at the lowest levels of income. These patterns were confirmed by Duncan et al. (1998), who related completed schooling and non-marital fertility to stage-specific childhood income and a substantial set of demographic controls.

With a few notable exceptions, none of the empirical studies linking economic disadvantage and child development has employed techniques to eliminate biases associated with the omission of unmeasured factors such as parental ability, mental health or altruism in putting the needs of their children's development before their own.

Using the Panel Study of Income Dynamics (PSID) and the National Longitudinal Survey of Youth (NLSY), Mayer (1997) provides a set of tests for omitted-variable bias, including the addition of measures of parental income after the measurement of the child outcome as well as using only those components of parental income that are fairly independent of the actions of the family. In the first case, her argument is that future income cannot have caused the prior outcome, so that its inclusion adjusts for unmeasured characteristics of the parents. The addition of future income almost always produces a large reduction in the estimated effect of prior parental income, leading her to conclude that much of the estimated effect of income in the literature is spurious.

In the second case, her argument is that the level of income components such as welfare and earnings (as well as the child outcomes under study) may reflect the effects of important

unmeasured parental characteristics. If components such as asset income are less affected by these unmeasured parental characteristics, then their coefficients ought to provide a better gauge of "true" income effects. Following this procedure, Mayer finds small and often insignificant coefficients on these income components.

As Mayer herself points out, these procedures are not without their problems. If families anticipate future income changes and adjust their consumption accordingly, and the consumption changes benefit or hurt children, then future income does indeed play a causal role. The likely measurement error in income sources such as dividends and interest will impart a downward bias in their coefficients. Moreover, since interest and dividends are almost universally absent from the income packages of families at or below the poverty line, these exogenous income sources are not useful for estimating the impact of income increments to low income families.

Shea (1997) estimates the impact of parents' income on their children's labor market outcomes by focusing on parental income variation due to factors that arguably reflect luck (i.e. union, industry and job loss experience). He uses children from the PSID and performs two-stage least squares regressions of children's income on demographic characteristics, fathers' observable skills and measures of parents' income using father's union, industry and job loss variables as instruments for parents' income. He finds that changes in parents' income due to luck have at best a negligible impact on children's human capital. As he points out, some of his instruments may be correlated with unobserved ability and he argues that this may bias his coefficients upward. However, in the case of union status and industry it is unclear whether the bias is upward or downward.

Blau (1999) uses data from the National Longitudinal Survey of Youth to estimate a number of models relating income and other aspects of parental family background to children's

ability and achievement test scores as well as behavior problems. These outcomes are assessed for most children in middle childhood. Among his approaches are a set of family fixed-effect models that relate within-family differences in test scores to within-family differences in the income histories of the individual children. He employs two alternative measures of income: income (and, for some models, wage rates) during the calendar year prior to the developmental assessment, and average household income of the mother over all years from 1979 to 1991 in which the data were available. In general, he finds small and insignificant effects of current income and larger (though still modest) effects of long-run income.

While Blau addresses the omitted variable bias and endogeneity issues, his measures of income fail to recognize the possibility that the timing of parental income during childhood may be important. He implicitly assumes that families can smooth income perfectly and that parental income during early childhood is equivalent (in terms of affecting child development) to income received in other stages of childhood.

Duncan et al. (1998) use data from the Panel Study of Income Dynamics to relate completed schooling and non-marital childbearing to average incomes in early and middle childhood and adolescence. Among their approaches are sibling models that relate differences in completed schooling to sibling differences in stage-specific family income. Their results confirm those of their individual-based models in that family income in early childhood had the strongest association with completed schooling.

Their sibling study leaves many unanswered questions. They use a linear specification, which ignores the possibility that income may have a larger effect for families at the lower end of the income distribution. Also, they only use 328 sibling pairs, which limits the precision of their estimates and the possibilities for conducting sensitivity analyses.

For example, income may be allocated differently in families that remain together over time (intact families) than in families that change because of divorce or remarriage (Lundberg and Pollak, 1996). This is because parents undergoing fundamental changes may themselves have changing preferences for their children's (and, in some cases, step-children's) attainments. In addition, intact families may be more effective in implementing their plans (e.g. saving for their children's college education). These considerations led us to believe that income for intact families may have a stronger effect on their children's attainment than the income in non-intact families.

From a theoretical perspective, an important implication that emerges from the child development literature is the fact that family income may have a distinct effect in different stages of childhood. Summarizing the developmental literature, Berk (1997) defines stages as "qualitative changes in thinking, feeling, and behaving that characterize particular time periods in development." She adds that "the stage concept assumes that children undergo periods of rapid transformation as they step up from one step to the next, followed by plateaus in which they stand solidly within a stage."

Freud, Erikson, and Piaget, among others, posit different theories of when these stages occur and what happens in them. While detailing each of these theories is beyond the scope of this paper, it is important to note that all imply that inputs to children's development may be consequential for development in some periods but not others. Thus, parental income may matter more in some stages of childhood than in others.

In particular, parental income may be important during early childhood if it helps buy goods and services that are crucial for successful cognitive and socioemotional development. For example, books and toys are key ingredients for a stimulating home learning environment, which

has been linked to children's early cognitive development. Smith et al. (1997) find that the quality of the home environment –its opportunities for learning, the warmth of the mother-child interactions, and the physical conditions of the home– accounts for a substantial portion of the effects of family income on cognitive outcomes of young children.

Family income during this period may also be important if it improves the parents' ability to raise their children by reducing stress in their relationship (Elder, 1999). Early childhood is also the period in which the scope for parental influence is the greatest. Afterwards, other contexts (such as peers, schools, neighborhoods, etc.) consume increasing amounts of children's time and energies.

Family income in adolescence may also have an effect on completed schooling because it affects families' abilities to afford college expenses. If parents cannot smooth perfectly their income streams, then income during adolescence may be particularly important for post-secondary schooling. None of the achievement studies reported in Duncan and Brooks-Gunn (1997) found large effects when using exclusively adolescence-based income measures, although these studies did not control for parental income earlier in life.

III – Theoretical Framework

The theoretical underpinnings of this paper come from the work of Becker and Tomes (1976), which provides a framework based on maximizing behavior that incorporates concern by parents for children as expressed in altruism towards children, investments in the human capital of children and the demand for children. This section briefly describes a simplified version of the Becker-Tomes framework relevant to our work.

We can think of parents as transmitting to their children some endowment (E), which is partly genetic and partly cultural, without the need to distinguish between these two forms of transmission. Apart from passing endowments to their children, parents also influence the adult earnings of their children through expenditures on their skills, health, learning, motivation and other characteristics.

For simplicity, assume two periods of life, childhood (t) and adulthood (t+1). Parents are assumed to derive utility from their own consumption in these two periods as well as from the utility that their children derive from their economic success as adults. Assuming no liquidity constraints, the parents are hypothesized to choose C^p_t, C^p_{t+1}, and S^p_t to solve the following maximization problem:

$$\text{Max} \quad U^p(C^p_t) + \delta U^p(C^p_{t+1}) + \delta U^c(Y^c_{t+1})$$

$$\text{s.t.} \quad \text{(i)} \; C^p_t + C^p_{t+1}/(1+r) + S^p_t \leq Y^p_t + Y^p_{t+1}/(1+r)$$

$$\text{(ii)} \; Y^c_{t+1} \leq f(S^p_t, E)$$

where U^p represents the utility of parents; U^c represents the utility of children; δ is the inter-temporal rate of substitution; Y^p, C^p and S^p represent parental income, consumption, and expenditure on children's schooling, respectively, in periods t or t+1. The variable Y^c_{t+1} represents the income of the child in period t+1. The market interest rate is denoted by r.

The optimal level of schooling expenditure is a function of r, E, and $Y^p_t+Y^p_{t+1}/(1+r)$,

$$S_t^* = h(E, r, Y^p_t+Y^p_{t+1}/(1+r))$$

Notice that in this case the optimal level of expenditures on children's schooling depends on the present value of the parents' income stream (i.e. $Y^p_t+Y^p_{t+1}/(1+r)$). The timing of income

does not matter since parents will be able to borrow against their future income to finance investments in their children's education.

Now consider the situation in which the parents are liquidity constrained. In this case, the parents choose C^p_t, C^p_{t+1}, and S^p_t to solve the following maximization problem:

Max $U^p(C^p_t) + \delta U^p(C^p_{t+1}) + \delta U^c(Y^c_{t+1})$

s.t. (i) $C^p_t + S^p_t \leq Y^p_t$; $C^p_{t+1} \leq Y^p_{t+1}$

(ii) $Y^c_{t+1} \leq f(S^p_t, E)$

The optimal level of expenditure on schooling will now be a function of r, E, Y_t and Y_{t-1},

$S_t^* = h(E, r, Y_t, Y_{t-1})$

Notice that parents can no longer borrow from the future to finance the education of their children and hence the timing of income does matter, i.e. in this case S_t^* depends on Y^p_t and Y^p_{t+1} separately.

IV – Empirical specification

Drawing on Griliches (1979), we start our discussion of the empirical specification used in analyzing sibling data with a simple equation containing a single family income variable,

(1) $SCHILD = \alpha + \beta\ FAMINC + \gamma F + \varepsilon$

where SCHILD is years of child's schooling, FAMINC represents family income of the child, and F is a set of family characteristics. Estimation of β will be biased if key components of F are either not measured or measured with error, and if F and FAMINC are correlated.

For example, suppose that we lack an explicit measure of parental mental health. Since this variable is likely to affect positively both family income and children's schooling, the OLS

estimate of β will tend to be biased upward. On the other hand, the estimate of β could be biased downward if, for example, one of the two parents decides to spend time caring for the child rather than in the labor market, in order to increase their children's schooling, and parental time allocation is not included in the estimating equation.

As Griliches points out, if the omitted F variables are persistent and siblings have the same level of F, then estimating β from within-family data would eliminate the bias in β. It is unlikely that all components of F are time-invariant but if, as seems reasonable, sibling differences in the time-varying components of F are largely independent of income differences, then estimating equation (1) using sibling data will produce unbiased coefficients of β.

To understand the way sibling data might help deal with the issues at hand, we divide the family variable F into two components: FPERM, which represents fixed family characteristics (e.g., race or mother's intelligence); and FTV, which represents family characteristics that vary over time (for example, family size).

FPERM is the same for all siblings whereas FTV -even though it is a family-level variable- will vary across siblings. Consider time-varying family size. At any given point, a family has only one value for this variable but different children of the same family may have different family sizes at any given age. So for example, a first-born child is likely to live in a family of small size during childhood relative to a child from the same family born a few years later. We account for this with:

$$(2) \qquad SCHILD_{ij} = \alpha + \beta\, FAMINC_{ij} + \gamma_1 FPERM_j + \gamma_2 FTV_{ij} + \varepsilon_{ij}$$

where i is a sub-index that denotes the individual sibling and j denotes the family. After taking averages within each family and subtracting them from (2), we get:

$$(3) \qquad SCHILD_{ij} - SCHILD_{.j} = \beta[FAMINC_{ij} - FAMINC_{.j}] + \gamma_2[FTV_{ij} - FTV_{.j}] + [\varepsilon_{ij} - \varepsilon_{.j}]$$

where $SCHILD_j$, $FAMINC_j$, FTV_j and ε_j are family averages for SCHILD, FAMINC, FTV and ε respectively. The family fixed effect $FPERM_j$ gets differenced out. The time-varying family effect (FTV_{ij}) remains because it is different for every child.

Even though the fixed effects methodology does not allow us to control for unobserved family characteristics that vary over time, it does allow us to control for a potentially important set of variables that are likely to bias the OLS estimates: unobserved persistent family characteristics. Sibling differences in the unobserved time-varying family characteristics will bias the estimate of β only if they are correlated with both sibling differences in income and sibling differences in completed schooling.

Allowing for the effect of family income to vary by stage of childhood provides us with our estimating equation:

$$(4) \quad SCHILD_{ij} - SCHILD_j = \beta_1[FAMINC1_{ij} - FAMINC1_j] + \beta_2[FAMINC\,2_{ij} - FAMINC2_j] + \beta_3[FAMINC3_{ij} - FAMINC3_j] + \beta_4[FAMINC4_{ij} - FAMINC4_j] + \beta_5[FTV_{ij} - FTV_j] + [\varepsilon_{ij} - \varepsilon_j]$$

where FAMINC1, FAMINC2, FAMINC3, FAMINC4 represent average family income during early childhood (0-4 years), early middle childhood (5-8 years), pre-adolescence (9-12 years), and adolescence (13-16 years), respectively.

V - Data

We use data from the 1968-1996 waves of the Panel Study of Income Dynamics, a longitudinal survey of U.S. households. Since 1968 the PSID has followed, interviewed annually, processed, analyzed, and disseminated information from a representative sample of about five thousand families (Hill, 1992). Splitoff families are formed when children leave home, when couples divorce, and when more complicated changes break families apart. This procedure produces an unbiased sample of families each year as well as a continuously representative sample of children born into families each year (Fitzgerald et al., 1998a, 1998b).

We use 1,364 families with children born between 1968 and 1976 and present in the PSID between birth and at least age 20. In most of the analysis, we restrict attention to families with more than one child. Given the cohort range, siblings cannot be more than 9 years apart in age. Individuals were defined to be siblings if they co-resided during all overlapping years in their first 15 years of childhood. Our sibling sample contains 863 children grouped in 391 families. Descriptive statistics for our complete and sibling samples are presented in Tables A1 and A2.

Our outcome measure -- completed years of schooling -- was measured when each child was age 20.[1] We would have liked to look at completed schooling at a later age but sample size considerations prevented us from doing so.[2]

Our income measure is the total pre-tax income of all family members, inflated to 1993 price levels using the CPI-UX1, and averaged over all of the years within the given childhood stage under consideration. In this and all other instances of stage-specific measures, the stages span ages 0-4, 5-8, 9-12, and 13-16[3].

Some of our analyses include control variables for the age of the mother at the time of the child's birth, family structure, maternal employment and residential mobility. The family structure measures constructed for each sibling took the form of dummy variables indicating whether the child was born into an intact family, and childhood-stage-specific measures of whether the child's parents divorced or remarried. Maternal employment is captured by stage-specific measures of the number of years in which the mother worked 1,000 or more hours. Residential mobility is measured with stage-specific counts of the number of years in which the family reported a residential move.

Our rationale for including these controls is that they represent conditions and events that may produce changes in income and at the same time have independent effects on child outcomes. Thus, failure to include them would lead to omitted variable bias. On the other hand, some of them (i.e. mother's labor supply) may be considered endogenous and hence a source of bias. For this reason, we ran all specifications with and without these controls.

VI – Results

Although our ultimate intent is in estimating the effect of stage-specific childhood income in schooling, we begin with a simpler model that uses only one childhood income measure -- average family income during the entire childhood. The first panel of Table 1 presents the results of a regression of years of children's completed schooling at age 20 on childhood income measured in tens of thousands of dollars. The OLS estimates are positive and highly significant (at the 1% level)[4]. However, magnitudes are very small, on the order of 0.15-0.20, which, if causal, implies that augmenting a family's income by $10,000 annually for the first 15 years of a child's life will increase the child's education by about one-fifth of a year. The family

fixed-effects (FE) coefficients are smaller (in the range of 0.04-0.12) and estimated much less precisely.[5] The imprecision of the fixed effects estimates is not surprising since there is little difference in 15-year average income between siblings.

The second panel in Table 1 presents results from a regression of years of schooling by age 20 on the log of whole-childhood income. The results for OLS coefficients are again positive and highly significant (at the 1% level) and the fit of the model to the data is higher than it was for the linear specification. The magnitudes are in the order of 0.75, which, if causal, implies that providing a family with a 2.7 fold increase in their income annually for the first 15 years of their children's life will increase their children's education by about 0.75 years. The fixed effects (FE) coefficients are sensitive to the presence of control variables and estimated imprecisely.

Table 2 summarizes our results using stage-specific income measures. Complete results are presented in Table A5. In both the linear and log specification, the OLS suggests that income during the fourth stage of childhood (13-16 years) has a positive and significant effect on achievement.[6] In our conceptually-preferred family fixed-effects models, income during the first stage of childhood is significant in the linear specification (with and without controls) and in the log specification (with controls only).[7] The linear specification suggests that a $10,000 increase in average annual family income during the first four years of a child's life is associated with an increase of 0.11-0.16 years of completed schooling. The log specification with controls suggests that a 2.7-fold increase in the average annual family income during the first four years of childhood raises the years of schooling by 0.47 years.

Given the interest in the very early period of life, we broke down the first stage of childhood into two sub-stages (0-2 and 3-4 years) and found that the positive and significant effect only appears in the first sub-stage (0-2 years). However, we were not able to reject the

hypothesis that the two coefficients (0-2 and 3-4) were equal to each other. For details, see Table A6.

VII - Extensions

Table 3 presents the results of various extensions of the log income specification shown in Table 2. We first restricted the sample to all children who were born into a two-parent family and whose parents remained married for at least 15 years after their birth. This restriction is motivated by the idea that families undergoing fundamental changes may themselves have changing preferences and abilities to implement plans for their children's (and, in some cases, step-children's) attainments. The results suggest that income during the first stage of childhood has a positive and significant effect on completed schooling, with the coefficients somewhat larger than in the larger sample of all siblings.

The basic fixed-effects specification uses all families with two or more siblings regardless of the age difference between children. A potential problem is that siblings close in age will have very similar stage-specific average incomes. For these families, there may be very little variation to exploit using the FE methodology and this may explain the lack of significance of income during some stages of childhood. To examine this issue, we ran the log specification regressions of Table 2 on a sub-sample of families whose children were three or more years apart in age from each other.[8]

Restricting the sample in this way more than doubles the explained variance of the model and confirms that income during the first stage of childhood has a positive and significant effect on completed schooling. This sample restriction also boosts the estimated impact of income

during the first and fourth stage of childhood (13-16 years old).[9] In this case, family income during adolescence becomes significant.

Estimates in the final pair of columns in Table 4 are based on a specification that combines the two previous restrictions by using a sub-sample of intact families with siblings three or more years apart. As with the larger three-years-apart sample, both the first and fourth stage income coefficients are positive and statistically significant.

We also investigated whether income had different effects on the educational attainment of boys vs. girls and blacks vs. non-blacks. We found no evidence of these kinds of interactions. Finally, we estimated spline regressions to assess whether the effect of income is different for families below a certain threshold ($15,000) than for those above this threshold. Standard errors here were too large to draw any conclusions.

VIII - Discussion

In this paper, we used sibling data from the Panel Study of Income Dynamics (PSID) to assess the impact of stage-specific family childhood income on completed years of schooling. We used family fixed effects techniques to eliminate biases associated with the omission of unmeasured persistent family characteristics.

Our results suggest that family income during childhood has a positive effect on completed schooling. Furthermore they indicate that the timing of income is important. In particular, our main results suggest that income during the first stage of childhood (0-4 years old) has a positive effect on completed schooling. The magnitude of the coefficient ranges from about 0.5 to 1.0, suggesting that a 2.7-fold increase in parental income when the child is 0-4 years old is associated with an increase of between half a year and one full year of schooling. Our

explanation for the significance of this result is that early childhood is the period in which the scope for family influence is the greatest. This is consistent with evidence from the child development literature pointing to early childhood as a critical developmental stage.

Some results (OLS and Fixed Effects with widely spaced children) suggest that family income during adolescence (13-16 years old) may also be an important determinant of educational attainment. This suggests that shocks to parental income during adolescence may affect the decision of attending college. Restricting the sample to intact families also produces larger estimated impacts of early childhood, which may reflect a greater impact of income in families where parents share long-run goals.

From a policy perspective, the results suggest that schooling is a possible channel through which income inequality can be transmitted from one generation to the next one. They also suggest that economic deprivation, especially early in life, may have long-run consequences for the well-being of children.

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Table 1 - Effect of 15-year Average Childhood Income on Years of Schooling at Age 20.

Regression 1: Linear specification, with childhood income measure	**OLS Regression**		**Fixed Effects**	
	No Controls	**With Controls**	**No Controls**	**With Controls**
15-year average family income, age 0-15 (in $10,000's)	.209***	.129***	.042	.117
	(.024)	(.029)	(.098)	(.123)
Observations	869	867	869	869
R-squared	.125	.198		
Within R-squared			.000	.062

Regression 2: Log specification, with childhood income measure	**OLS Regression**		**Fixed Effects**	
	No Controls	**With Controls**	**No Controls**	**With Controls**
15-year average family income, age 0-15 (in logs)	.819***	.696***	.228	.672
	(.084)	(.112)	(.444)	(.554)
Observations	869	867	869	869
R-squared	.154	.220		
Within R-squared			.001	.063

- Standard errors are in parentheses. ***: signif at 1% level; **: signif at 5% level; *: signif at 10% level.
- Huber-White robust standard errors were calculated for OLS regressions.
- Control variables for the Fixed Effects models include: Mother's age at birth of child, average family size for each stage, dummy indicating whether born into 2-parent family, stage-specific dummies indicating whether parents ever divorced, whether they ever remarried, whether family ever moved and whether mother worked more than 1000 hours a year.
- Control variables for the OLS models include the same as in the Fixed Effects models plus sex and race dummy variables, mother's years of education and dummy variables indicating whether family ever lived in the south.

Table 2 - Effects of Stage-Specific Childhood Income on Years of Schooling at age 20

	OLS Regression		Fixed Effects	
Regression 1: Linear Specification	**No Controls**	**With Controls**	**No Controls**	**With Controls**
Avg. family income, age 0-4 (in $10,000's)	.056	.004	.108*	.163**
	(.045)	(.047)	(.063)	(.075)
Avg. family income, age 5-8 (in $10,000's)	.057	.051	.075	.115*
	(.039)	(.040)	(.063)	(.068)
Avg. family income, age 9-12 (in $10,000's)	-.027	-.047	-.093*	-.056
	(.043)	(.046)	(.055)	(.058)
Avg. family income, age 13-16 (in $10,000's)	.116***	.104***	.006	.034
	(.027)	(.029)	(.045)	(.051)
Observations	863	861	863	863
R-squared	.137	.222		
Within R-squared			.015	.104

	OLS Regression		Fixed Effects	
Regression 2: Log Specification	**No Controls**	**With Controls**	**No Controls**	**With Controls**
Avg. family income, age 0-4 (in logs)	.126	-.001	.281	.469*
	(.145)	(.153)	(.201)	(.244)
Avg. family income, age 5-8 (in logs)	.234	.295*	.144	.334
	(.151)	(.169)	(.232)	(.258)
Avg. family income, age 9-12 (in logs)	-.133	-.197	-.226	-.122
	(.148)	(.155)	(.201)	(.226)
Avg. family income, age 13-16 (in logs)	.543***	.560***	.172	.230
	(.118)	(.127)	(.217)	(.264)
Observations	863	861	863	863
R-squared	.172	.251		
Within R-squared			.009	.098

- The sample consists of families with two or more siblings (863 siblings, 391 families).
- Standard errors are in parentheses. ***: signif at 1% level; **: signif at 5% level; *: signif at 10% level.
- Huber-White robust standard errors were calculated for OLS regressions.
- Control variables for the Fixed Effects models include: Mother's age at birth of child, average family size for each stage, dummy indicating whether born into 2-parent family, stage-specific dummies indicating whether parents ever divorced, whether they ever remarried, whether family ever moved and whether mother worked more than 1000 hours a year.
- Control variables for the OLS models include the same as in the Fixed Effects models plus sex and race dummy variables, mother's years of education and dummy variables indicating whether family ever lived in the south.

Table 3 - Extensions to Fixed Effects Models

	All families in sibling sample (repeated from Table 2)		Intact families only		Families with siblings 3 or more years apart		Intact families with siblings 3 or more years apart	
	No Controls	With Controls	No Controls	With Controls	No Controls	With Controls	No Controls	With Controls
Avg. family income, age 0-4 (in logs)	.281	.469*	.478*	.651*	.536**	.915***	.708**	.963**
	(.201)	(.244)	(.285)	(.346)	(.227)	(.282)	(.321)	(.386)
Avg. family income, age 5-8 (in logs)	.144	.334	-.445	-.390	.269	.270	-.293	-.306
	(.232)	(.258)	(.376)	(.429)	(.265)	(.298)	(.424)	(.490)
Avg. family income, age 9-12 (in logs)	-.226	-.122	-.348	-.346	-.081	-.051	-.294	-.226
	(.201)	(.226)	(.302)	(.345)	(.227)	(.261)	(.339)	(.389)
Avg. family income, age 13-16 (in logs)	.172	.230	.220	.319	.536**	.843**	.563	.885*
	(.217)	(.264)	(.315)	(.366)	(.252)	(.323)	(.377)	(.454)
Observations	863	863	566	566	467	467	308	308
Within R-squared	.009	.098	.018	.065	.049	.212	.059	.164

- Standard errors are in parentheses. ***: signif at 1% level; **: signif at 5% level; *: signif at 10% level.
- Control variables include: Mother's age at birth of child, average family size for each stage, stage-specific dummies indicating whether parents ever divorced, whether they ever remarried, whether family ever moved and whether mother worked more than 1000 hours a year.

Table A1 - Descriptive Statistics

	Whole sample		Sample restricted to families with 2 or more siblings		p-value differences of means between whole sample and sibling sample	p-value differences of means between sibling sample and non-sibling sample
	Mean	Std.Dev.	Mean	Std. Dev.		
Income variables						
Avg. family Income, age 0-4 (in $10,000s)	3.29	1.82	3.39	1.61	0.015	0.034
Avg. family Income, age 5-8 (in $10,000s)	3.71	2.27	3.86	1.99	0.005	0.009
Avg. family Income, age 9-12 (in $10,000s)	4.04	2.92	4.14	2.37	0.267	0.187
Avg. family Income, age 13-16 (in $10,000s)	4.39	3.31	4.57	2.88	0.044	0.036
Sibling-specific variables						
Completed years of education by age 20	11.98	1.25	12.06	1.18	0.032	0.015
Whether born into 2-parent family	0.82	0.39	0.86	0.34	0.000	0.000
Mother's age at birth of child	24.35	5.66	24.13	4.37	0.242	0.124
Whether ever divorced, age 0-4	0.09	0.29	0.07	0.26	0.000	0.004
Whether ever divorced, age 5-8	0.11	0.31	0.09	0.28	0.000	0.004
Whether ever divorced, age 9-12	0.09	0.28	0.08	0.27	0.120	0.226
Whether ever divorced, age 13-16	0.09	0.28	0.08	0.27	0.052	0.127
Whether ever remarried, age 0-4	0.09	0.29	0.10	0.29	0.921	0.973
Whether ever remarried, age 5-8	0.06	0.25	0.05	0.22	0.001	0.008
Whether ever remarried, age 9-12	0.06	0.24	0.03	0.18	0.000	0.000
Whether ever remarried, age 13-16	0.06	0.24	0.03	0.18	0.000	0.000
Number of years moved, 0-4	1.79	0.87	1.81	0.84	0.028	0.376
Number of years moved, 5-8	0.92	0.96	0.78	0.88	0.000	0.000
Number of years moved, 9-12	0.73	0.95	0.55	0.82	0.000	0.000
Number of years moved, 13-16	0.76	1.01	0.61	0.92	0.000	0.000
Number of years mom worked 1000+ hrs, 0-4	1.15	1.36	1.00	1.27	0.000	0.000
Number of years mom worked 1000+ hrs, 5-8	1.42	1.59	1.27	1.53	0.000	0.000
Number of years mom worked 1000+ hrs, 9-12	1.79	1.69	1.75	1.68	0.126	0.336
Number of years mom worked 1000+ hrs, 13-16	2.15	1.71	2.12	1.71	0.186	0.425
Whether ever lived in South	0.53	0.50	0.48	0.50	0.000	0.000
Average family size, age 0-4	4.43	1.78	4.49	1.78	0.133	0.147
Average family size, age 5-8	4.70	1.58	5.01	1.53	0.000	0.000
Average family size, age 9-12	4.75	1.49	5.16	1.47	0.000	0.000
Average family size, age 13-16	4.62	1.38	5.00	1.33	0.000	0.000
Family-specific controls						
Whether female,1=female,0=male	0.48	0.50	0.50	0.50	0.174	0.300
Whether black,1=black 0=white	0.41	0.49	0.34	0.48	0.000	0.000
Mother's years of education	11.75	2.32	11.94	2.28	0.001	0.001
Number of observations	1836	-	863	-		-

Table A2 – Some additional descriptive statistics of main variables

(i) Distribution of Completed Years of Schooling by age 20

Years of schooling by age 20	Frequency	Percentage	Cumulative perc. (%)
7	1	0.1%	0.1%
8	10	1.2%	1.3%
9	24	2.8%	4.1%
10	45	5.2%	9.3%
11	91	10.5%	19.8%
12	434	50.3%	70.1%
13	177	20.5%	90.6%
14	74	8.6%	99.2%
15	7	0.8%	100.0%
Total	863	100.0%	

(ii) Family distribution according to the number of siblings in the data set

Number of siblings	Frequency	Percentage
1	918	50%
2	651	35%
3	208	11%
4	53	3%
6	6	0%
Total	1836	100%

Table A3 - Effects of Stage-Specific Childhood Income on Years of Schooling at age 20
Results from OLS, Fixed Effects and Random Effects Specification
3 stages of childhood

	OLS Regression		Fixed Effects	
Regression 1: Linear Specification	**No Controls**	**With Controls**	**No Controls**	**With Controls**
Avg. family income, age 0-5 (in $10,000's)	.107**	.043	.133*	.172**
	(.047)	(.047)	(.069)	(.081)
Avg. family income, age 6-10 (in $10,000's)	-.024	-.015	-.022	.012
	(.036)	(.039)	(.066)	(.069)
Avg. family income, age 11-15(in $10,000's)	.124***	.091***	-.037	-.007
	(.025)	(.029)	(.054)	(.062)
Observations	869	867	869	869
R-squared	.132	.201		
Within R-squared			.0084	.0693

	OLS Regression		Fixed Effects	
Regression 2: Log Specification	**No Controls**	**With Controls**	**No Controls**	**With Controls**
Avg. family income, age 0-5 (in 10,000's)	.282*	.104	.348	.549**
	(.159)	(.161)	(.227)	(.271)
Avg. family income, age 6-10 (in 10,000's)	-.063	.060	-.190	.052
	(.154)	(.172)	(.264)	(.280)
Avg. family income, age 11-15 (in 10,000's)	.544***	.471***	.056	.182
	(.112)	(.133)	(.221)	(.258)
Observations	869	867	869	869
R-squared	.163	.225		
Within R-squared			.0065	.0686

Robust standard errors in parentheses (for OLS regressions)
***: signif at 1% level, **: signif at 5% level; *: signif at 10% level.
- Control variables for the Fixed Effects models include: Mother's age at birth of child, average family size for each stage, dummy indicating whether born into 2-parent family, stage-specific dummies indicating whether parents ever divorced, whether they ever remarried, whether family ever moved and whether mother worked more than 1000 hours a year.

Table A4 - Effects of Stage-Specific Childhood Income on Years of Schooling at age 20
Results from OLS, Fixed Effects and Random Effects Specification
5 stages of childhood

	OLS Regression		Fixed Effects	
Regression 1: Linear Specification	**No Controls**	**With Controls**	**No Controls**	**With Controls**
Avg. family income, age 0-3 (in $10,000's)	.018	-.023	.084	.113*
	(.044)	(.044)	(.058)	(.067)
Avg. family income, age 4-6 (in $10,000's)	.102**	.063	.058	.091
	(.048)	(.046)	(.058)	(.062)
Avg. family income, age 7-9 (in $10,000's)	-.028	-.017	.023	-.005
	(.042)	(.945)	(.055)	(.059)
Avg. family income, age 10-12(in $10,000's)	.008	-.013	-.082	-.056
	(.044)	(.043)	(.053)	(.058)
Avg. family income, age 13-15(in $10,000's)	.102***	.096***	.003	.037
	(.029)	(.030)	(.046)	(.052)
Observations	871	869	871	871
R-squared	.136	.227		
Within R-squared			.013	.100

	OLS Regression		Fixed Effects	
Regression 2: Log Specification	**No Controls**	**With Controls**	**No Controls**	**With Controls**
Avg. family income, age 0-3 (in logs)	.075	-.033	.244	.327
	(.135)	(.140)	(.175)	(.217)
Avg. family income, age 4-6 (in logs)	.256*	.203	.078	.244
	(.149)	(.153)	(.209)	(.227)
Avg. family income, age 7-9 (in logs)	-.022	-.016	-.011	-.110
	(.153)	(.162)	(.208)	(.231)
Avg. family income, age 10-12 (in logs)	-.018	-.035	-.182	-.085
	(.149)	(.151)	(.187)	(.214)
Avg. family income, age 13-15 (in logs)	.476***	.510***	.166	.308
	(.125)	(.131)	(.195)	(.231)
Observations	871	869	871	871
R-squared	.169	.255		
Within R-squared			.008	.096

Robust standard errors in parentheses (for OLS regressions)
***: signif at 1% level; **: signif at 5% level; *: signif at 10% level.

Table A5 – Effect of Stage-Specific Childhood Income on Years of Schooling at age 20
All coefficients reported. Log specification.

	OLS Regression		Fixed Effects	
	No Controls	With Controls	No Controls	With Controls
Avg. family income, age 0-4 (in logs)	.126	-.001	.281	.469*
	(.145)	(.153)	(.201)	(.244)
Avg. family income, age 5-8 (in logs)	.234	.295*	.144	.334
	(.151)	(.169)	(.232)	(.258)
Avg. family income, age 9-12 (in logs)	-.133	-.197	-.226	-.122
	(.148)	(.155)	(.201)	(.226)
Avg. family income, age 13-16 (in logs)	.543***	.560***	.172	.230
	(.118)	(.127)	(.217)	(.264)
Whether born into 2-parent family		.089		.045
		(.135)		(.204)
Mother's age at birth of child		.004		.009
		(.012)		(.039)
Whether ever divorced, age 0-4		.176		-.006
		(.164)		(.225)
Whether ever divorced, age 5-8		-.142		-.024
		(.187)		(.297)
Whether ever divorced, age 9-12		.154		-.101
		(.184)		(.321)
Whether ever divorced, age 13-16		-.048		-.494*
		(.196)		(.267)
Whether ever remarried, age 0-4		.005		.055
		(.141)		(.215)
Whether ever remarried, age 5-8		-.236		.053
		(.218)		(.320)
Whether ever remarried, age 9-12		-.605**		-.428
		(.240)		(.405)
Whether ever remarried, age 13-16		-.137		.171
		(.243)		(.334)
Number of years moved, age 0-4		-.014		.102
		(.048)		(.078)
Number of years moved, age 5-8		.069		-.097
		(.050)		(.109)
Number of years moved, age 9-12		.040		.084
		(.062)		(.105)
Number of years moved, age 13-16		-.149**		.111
		(.061)		(.089)
Number of years mom worked 1000+ hrs, 0-4		.069*		.037
		(.037)		(.058)
Number of years mom worked 1000+ hrs, 5-8		-.046		-.118*
		(.039)		(.067)
Number of years mom worked 1000+ hrs, 9-12		.022		-.022
		(.037)		(.072)
Number of years mom worked 1000+ hrs, 13-16		-.006		-.096
		(.032)		(.062)
Average family size, age 0-4		.099**		-.007
		(.038)		(.058)
Average family size, age 5-8		-.219***		-.246**
		(.074)		(.119)
Average family size, age 9-12		.093		.088

		(.084)		(.119)
Average family size, age 13-16		-.003		-.111
		(.066)		(.114)
Whether ever lived in South		-.080		
		(.093)		
Whether female,1=female,0=male		.293***		.343***
		(.071)		(.083)
Whether black,1=black 0=white		.180		
		(.115)		
Mother's years of education		.068***		
		(.025)		
Constant	11.099***	10.300***	11.630***	12.118***
	(.127)	(.448)	(.523)	(1.389)
Observations	863	861	863	863
R-squared	.172	.251		

Robust standard errors in parentheses. ***: signif at 1% level; **: signif at 5% level; *: signif at 10% level.

Table A6 - Effects of Stage-Specific Childhood Income on Years of Schooling at age 20
Results from OLS, Fixed Effects Specification
Early childhood stage (0-4 years) broken into two stages (0-2 and 3-4)

	OLS Regression		Fixed Effects	
Regression 1: Linear Specification	**No Controls**	**With Controls**	**No Controls**	**With Controls**
Avg. family income, age 0-2 (in logs)	.036	.007	.082	.111*
	(.042)	(.045)	(.054)	(.060)
Avg. family income, age 3-4 (in logs)	.028	.004	.027	.041
	(.040)	(.042)	(.054)	(.057)
Avg. family income, age 5-8 (in logs)	.049	.046	.063	.108
	(.039)	(.040)	(.063)	(.068)
Avg. family income, age 9-12 (in logs)	-.005	-.033	-.084	-.059
	(.043)	(.046)	(.056)	(.059)
Avg. family income, age 13-16 (in logs)	.095***	.087***	-.020	.002
	(.029)	(.029)	(.043)	(.047)
Observations	876	874	876	876
R-squared	.133	.225		
Within R-squared			.014	.125

	OLS Regression		Fixed Effects	
Regression 2: Log Specification	**No Controls**	**With Controls**	**No Controls**	**With Controls**
Avg. family income, age 0-2 (in logs)	.141	.051	.227	.345**
	(.119)	(.133)	(.141)	(.160)
Avg. family income, age 3-4 (in logs)	-.036	-.069	.004	.106
	(.131)	(.144)	(.170)	(.188)
Avg. family income, age 5-8 (in logs)	.241	.304*	.074	.317
	(.151)	(.170)	(.233)	(.258)
Avg. family income, age 9-12 (in logs)	-.074	-.152	-.192	-.119
	(.147)	(.155)	(.203)	(.228)
Avg. family income, age 13-16 (in logs)	.500***	.521***	.103	.199
	(.117)	(.125)	(.218)	(.259)
Observations	876	874	876	876
R-squared	.171	.256		
Within R-squared			.009	.124

Robust standard errors in parentheses (for OLS regressions)***: signif at 1% level; **: signif at 5% level; *: signif at 10% level.

[1] If this information was missing at age 20, we assigned the value of completed schooling when the child was 21, and if also missing, we took the value when the child was 19.

[2] Given the fixed observation period, for every year we increase the age at which completed schooling is measured, we lose a cohort of children from our sample.

[3] We decided to use four stages of childhood for both theoretical and empirical reasons. On the theoretical side, our reading of the child development literature suggests that three stages may be too crude to capture the relevant transitions that a child typically goes through during his/her childhood. In deciding between using four or five stages, we relied on empirical considerations. Using five stages allows us to exploit more of the within family variation but is also more prone to measurement error given that income averages are computed over a smaller number of years. Furthermore, four stages provided a better fit (i.e. higher R^2 in all basic specifications) to the data than five stages. Tables A3 and A4 display results from the basic specifications, using three and five stages of childhood, respectively.

[4] Given that there are several observations for each family, robust standard errors were calculated. The procedure employed uses the Huber/White sandwich estimator of variance instead of the traditional calculation. It allows observations not to be independent within families (i.e. for siblings in the same family), although they must be independent between families.

[5] We compared the results of the OLS and fixed effects specifications with the ones obtained from the more efficient random effects model. This estimator is a weighted matrix average of the within (FE) and the between estimators, but is consistent only under the assumption that the family effect is uncorrelated with the regressors. Coefficients and standard errors from the random effects models were very similar to their OLS counterparts, which led us to present only

the OLS results.

[6] When we use three stages of childhood (Appendix A4) and OLS, we find, in contrast to Duncan et al. (1998), that the income coefficient for the first stage of childhood is not significant. The sample used here contains three more cohorts of children than the sample used by Duncan et al. (1998). When we restrict our sample to intact families, we do get the same pattern found in Duncan et al. (1998), i.e. significant income coefficients for the first and third stages of childhood.

[7] The sample used in Table 2 consists of those children in families with two or more siblings (863 out of the original 1836 children). It is possible that this sample is not representative because families with only one child might treat that child differently than multiple parity families would treat their children. This is an issue for all sibling samples. However, for the particular question we examine here, we see no compelling reasons for this to bias our results upward.

[8] There is a potential endogeneity bias arising from the fact that parents with widely-spaced children may differ (e.g. be more planful) than other parents. The same regressions were also run but with the age difference restriction modified to 2 and 4 years. The income coefficient for the first stage of childhood remained significant across all specifications. For the fourth stage of childhood, the income coefficient was not significant when the age difference restriction was set at 2 years, but it was significant for 3 and 4 years.

[9] The fact that our estimates in this sub-sample have smaller standard errors and remain significant for the first stage of childhood represents additional evidence that there is indeed an effect in this stage of childhood. The fact that the magnitude of the coefficients (for the first and

fourth stages of childhood) increases substantially requires a separate explanation. We believe that a possible explanation is that families with children very close to each other (in age) may not be as able to respond to negative income shocks as families with children spread apart. For example, a family with two adolescents may have more difficulty sending both children to college than a family with two children further apart in years. This latter explanation would be consistent with our finding that the magnitude of the effect of income in the fourth stage is much larger for families with children that are further apart in age than for the whole sample of families.